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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How important is the vertical structure for the representation of aerosol impacts on the diurnal cycle of marine stratocumulus?" *by* I. Sandu et al.

Anonymous Referee #1

Received and published: 24 April 2009

It looks like you've put significant thought into your corrections and I'm sure the new draft will look much better. Here are some responses to your responses. I think another round of review is in order.

Comment p 5471 l 25: Thanks for clarifying your subsidence treatment. I don't think your proposed subsidence explanation change adds any new information. I actually liked the original wording of the last paragraph on p 5471 better. The key point that you are still missing from the text is that $w_s=D*z$ for $z<z^*$ and $w_s=D * z^*$ for $z>z^*$ where $z^*=600$m was chosen because it is the initial BL depth (if I understand your comment response correctly). Note that I've switched your terminology of zi to z^* because zi is

typically the time-dependent BL depth which (I think) is only equal to z^* at t=0...

I agree that fixing z^* makes the free-tropospheric T less likely to drift since it makes the free-tropospheric vertical advection tendency constant in time. I don't like your methodology, however, because your model has fundamentally different subsidence forcing depending on whether zi is above or below 600 m. When zi is below 600 m, rising zi causes w_s to increase, damping further zi increase. Above 600 m this feedback is turned off by using constant w_s. When I was using a similar subsidence forcing scheme in a diurnally varying MLM study, I found clear differences in model behavior depending on whether zi was above or below my z^*. I suspect that your subsidence forcing methodology is the reason why in Fig. 2 the profile w/ zi>600m has zi»600m while profiles w/ zi<600m don't vary as much... which is kind of troubling.

I don't think this issue is cause for rejecting your paper, however, because for each of your experiments the pristine and polluted test cases either both lie above or both lie below 600m in your Fig. 2 and the supplementary Fig. 1 you supplied me. I would ask that you make sure that switching above/below 600m isn't affecting your results for any particular time of day. I also think it is critical that you include some mention of this issue in your paper.

I agree that injecting large-scale information into a STBL model is a huge challenge and I don't think anybody knows how to do it right yet. You should check out Wyant et al (2009; Journal of Advancing Modeling Earth Systems) for a new twist on the weak temperature gradient approach.

I very much like your p 5472 l 9 correction and agree that the fact that the free troposphere drifts in the same way for polluted and pristine cases renders your conclusions insensitive to this drift. I'd encourage you to remove the word "slight" (1-3K isn't slight!) and would like to see (ie show the reviewers, don't add to your paper) plots showing pristine and polluted free tropospheric qv and theta on the same axis (or perhaps a difference plot) because it is hard to quantify how different these are from the plots provided. Also, I think you mean "a few tenTHs K" instead of "a few tens of K".

Comment p 5481 l 17

I like your change. Maybe you should mention that the domain-ave divergence is generally less than that from the LES and note how that would affect your results.

Comment p 5484 Sect 5.2

The point I was trying to make is that you omit a key physical process from your MLM which you know is critical to STBL aerosol response and which could be included via existent parameterizations. This makes your conclusion that MLMs are inappropriate for this kind of research misleading. I think it would be dishonest of you to publish your paper without either including a parameterization with the relevant physics or acknowledging that sedimentation effects are critical to STBL aerosol response and are missing from your study.

General Comment 1:

I agree in principle with your changes, but I think they all need the caviat that this is the result for your experiment and may not be true in general. In particular, I can imagine a nighttime-only case where using a MLM would be quite reasonable. Also, I think you mean "should" not "shall" in your sentence for the abstract.

Specific comments:

Most seem fine except:

Table 4 comment:

I don't see why including biases of all parameterizations, even those with huge bias, would be a logistical challenge. Also, perhaps it would be useful to include values computed from well-mixed times only either in addition to or instead of what you've done. My reason for suggesting this is that a parameterization which does well during non-well mixed times is doing so for the wrong reasons and may be masking poor

behavior during times when the BL is actually well mixed. I don't really care if you change these things, but I do think it would improve the paper.

General comments on your comments:

1. You keep saying that you don't use the Bretherton entrainment formulation because it's unclear which parameter values to use and you don't want to confuse readers. I think you should email Chris Bretherton and ask him for suggested parameter settings, then use just those settings in your study. Include a note in the text saying "using a2=... as recommended by C.S. Bretherton [personal communication]". I don't see how this would confuse readers and I can't imagine it taking very long to run the EML/MLM stuff if you already have his method coded up... Otherwise you should note in your paper that the MLM doesn't agree because it ignores sedimentation effects on entrainment...

2. I still get the feeling from your corrected excerpts that you are saying that MLMs are universally bad. I would like to see sentences like "MLMs perform poorly" changed to something like "MLMs perform poorly in our simulation".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5465, 2009.